

<https://helda.helsinki.fi>

---

## Essential Tensions in Scientific Discovery

Paavola, Sami

2001

---

Paavola , S 2001 , Essential Tensions in Scientific Discovery . in P Ylikoski & M Kiikeri (eds)  
, Explanatory Connections. Electronic essays dedicated to Matti Sintonen . <  
<http://www.valt.helsinki.fi/kfil/matti/paavola.pdf> >

---

<http://hdl.handle.net/10138/225750>

---

publishedVersion

---

*Downloaded from Helda, University of Helsinki institutional repository.*

*This is an electronic reprint of the original article.*

*This reprint may differ from the original in pagination and typographic detail.*

*Please cite the original version.*

# Essential Tensions in Scientific Discovery<sup>1</sup>

Sami Paavola

## Introduction

Discovery is very controversial topic in the philosophy of science. As Larry Laudan (1980) has written:

"It is difficult to find a problem area in the philosophy of science about which more nonsense has been talked and in which more confusion reigns than 'the philosophy of discovery'." (173)

Although this statement is surely rhetorical and a little bit exaggerated, there is still some truth in it today. Discovery is a very slippery concept, especially for philosophical analysis.

In this paper I will defend N. R. Hanson's ideas that processes of discovery are, however, a possible and highly important topic for philosophy of science. The area of discovery borders on a number of paradoxes but these paradoxes and tensions do not weaken any possible analyses, but rather they open up new possibilities to understand the dynamics of inquiry. One, classical paradox of inquiry is the so-called Meno-paradox (or learning paradox). I think that one implication of this paradox is that the interaction between theories and observations should be understood much more thoroughly than has hitherto been done. Charles Darwin's ideas about methodology fit very well into this kind of interactionist model.

## Hanson's challenge: from products to processes of discovery

Already in the 1950s and 1960s, N. R. Hanson challenged philosophers to investigate processes of discovery (Hanson 1961, 1972). He criticized philosophy of science, arguing that it concentrates too much on a "Logic of the Finished Research Report," i.e., it does not analyze how hypotheses and theories are discovered but only how

---

<sup>1</sup> This paper has originally been published as a web publication: Paavola, S. (2001). Essential Tensions in Scientific Discovery. In P. Ylikoski & M. Kiikeri (Eds.), *Explanatory Connections. Electronic essays dedicated to Matti Sintonen*.

already existing theories can be justified. Hanson himself was looking for "a Logic of Discovery". He emphasized that this logic of discovery does *not* mean for him a mechanical manual for making discoveries (Hanson 1961, 20-22); but even if there are no such manuals, processes of discovery can be analyzed by *conceptual* means, as e.g., Aristotle and Charles Peirce have done before Hanson. Hanson's claim was that there is much similarity how scientific research has been done previously and how it is done now although the areas of research themselves have changed tremendously. Scientists are now, as they were before, seeking explanations of phenomena, and it is possible to find similar patterns in this search (Hanson 1972, 2).

It could be argued that several designers of influential models in the philosophy of science in 20th-century were particularly interested in the *processes* of inquiry. For example, one area where Karl Popper's and Thomas S. Kuhn's -- in many respects quite dissimilar -- ideas concerning philosophy of science were similar was an emphasis on the dynamic processes of knowledge acquisition rather than on the logical structure of the finished products of science (Kuhn 1970, 1). But still, I think, the "Hanson's challenge" is current (cf. also Kaplan 1964, 10; Garrison 1988, 149-150; Sintonen 1990, 681, 687). Both Popper and Kuhn, in fact thought that processes of *discovery* are not proper subjects for a philosophical and conceptual analysis. Popper's ideas can be interpreted so that in the *broad* sense he was interested in the logic of discovery, i.e., he was seeking to explain how the process of inquiry proceeds (Gutting 1980). But in the more strict sense, Popper is a famous opponent of the idea that processes of discovery could be analyzed conceptually (e.g., Popper 1980, 32). Kuhn also maintained that there could be a *psychological* or a *sociological* analysis of discovery but not a logical one (Kuhn 1970).<sup>2</sup>

In recent years, however, philosophers have started, more, to analyze the processes of discovery. New conceptual models and tools have been developed which can be

---

<sup>2</sup> Still, this is not an either/or situation. There are a great many similarities in how Hanson, Popper, and Kuhn characterized the area of discovery. Hanson emphasized Peircean abduction (Hanson 1961, 1972), and there are many similarities in Peirce's and Popper's ideas concerning the process of inquiry (Niiniluoto 1978). Whereas Kuhn's analysis emphasized the meaning of *anomalies* (Kuhn 1970, 1977, 174); anomalies go very well with Peircean abduction. But still, it was only Hanson who explicitly maintained that the area of discovery could be analyzed with philosophical concepts.

used for this purpose, e.g., the interrogative model of inquiry and new conceptualizations of abductive inference (Hintikka 1985, 1998; Sintonen 1996a, 1999; Aliseda 1997, 2001; Magnani, Nersessian & Thagard 1999; Paavola 2001). The background supposition is that there is the area of *heuristics* that is not strictly rational (at least in the old sense) but neither is totally blind, and it is possible to develop conceptual and methodological (and even strictly logical) models for heuristic procedures (Hintikka 1985; Nickles 1989; Pera 1994; Aliseda 2001). This supposition about heuristics would suggest that conceptual structures and a historical perspective should not be seen as opposites, rather "grammar" should embrace historical perspective (Sintonen 2000).

Many other areas of research have taken the issue of discovery much more seriously than philosophy. There are various models, e.g., in education, in artificial intelligence, in cognitive sciences, and in business sciences that have been proposed to capture processes of discovery (see Engeström 1987; Nonaka & Takeuchi 1995; Bereiter, in press). In these areas it is felt that in order to genuinely understand modern "knowledge society," it is important to conceptualize dynamic processes of knowledge advancement and knowledge creation, and not just to analyze how already existing knowledge is justified or acquired. For example, learning, in genuine and deep sense, can be understood as analogous to processes of innovative inquiry. This means that learning is seen as a collaborative effort to advance knowledge and understand things more thoroughly, i.e., to *discover* something new. The model of *progressive inquiry* is based on the idea that various conceptual means that have been developed in philosophy of science and in cognitive science can be used to model the "epistemological infrastructure" of learning. (Hakkarainen 1998; Hakkarainen & Sintonen, in press; Paavola, Lipponen & Hakkarainen 2001)

## Paradoxes or dynamics of discovery

One reason it is difficult to understand discovery is the paradoxical nature of it. Many claims and requirements that seem to be controversial are often connected to the idea of discovery. There are lots of examples: discoveries are often described as sudden moments of insight, but on the other hand, they can be seen as a result of hard work

and "perspiration". Or; creativity can be seen as a result of a "divergent" thinking and playfulness, which can break constraints and boundaries set by old ways of thinking. But, on the other hand, creativity can be seen to be based on "convergent" thinking where it is important to know those constraints, which older theories and paradigms require. Discoveries are almost by definition something unique, but often, in the history of science, similar discoveries are made at the same time. It can also be claimed that in order to find something new, it is important to be able to assess various possibilities impartially and critically, but on the other hand, it seems that discoverers often highly emotionally defend their favorite ideas even without much evidence. Innovativeness seems also to require that things are seen from many perspectives, but on the other hand, it is important to have a firm ground which does not change continually. The acts of creation seem to be often individual achievements where previous barriers of thought are transcended. But, on the other hand, it seems that they are the result of social interaction where the individual achievements are almost inevitable results of those resources that culture offers.

It could be argued that creativity and discovery are by their nature concepts that border on paradox. In discovery you almost have to *try* to have your cake and eat it too! Various models and characterizations of discovery are instructive because they often try to avoid dichotomies by emphasizing the dynamic way of thinking. In discoveries it is *not* the case that one should choose between insights and hard work, or between tradition and innovation, or between individual and community, or between logic and emotion. In productive models of discovery, it is both of these.

It is, I think, in this non-dichotomous spirit, that Thomas S. Kuhn maintained that:

"very often the successful scientist must simultaneously display the characteristics of the traditionalist and of the iconoclast" (Kuhn 1977, 227; Sintonen 2000).

Popular models of creativity emphasize flexibility, open- mindedness and lack of prejudice (i.e., "divergent thinking") as basis for new innovations. But according to Kuhn, the history of science shows that, as a matter of fact, maybe more important for discoveries is that the research is firmly rooted in particular tradition in question (i.e., on "convergent thinking"). This is according to Kuhn, an "essential tension" in scientific research. Scientific advance is based on education that teaches how things

are seen and done in that particular research area at that particular time. And it is just this tradition-bound research that ultimately leads to situation where this tradition must also be broken. One has to know the tradition well enough to be able to break it with a better model. Kuhn particularly emphasizes the meaning of tradition (because popular stereotypes about creativity have often neglected this), but both aspects of the essential tension are needed:

"the productive scientist must be a traditionalist who enjoys playing intricate games by pre-established rules in order to be a successful innovator who discovers new rules and new pieces with which to play them" (Kuhn 1977, 237).

Often there is a division of labour at the group level; some individuals are more traditionalistic, and some more iconoclastic in the community (Kuhn 1977, 227-8).

In a similar manner, Howard E. Gruber has argued, that two seemingly opposed approaches to creativity, i.e., creativity as sudden moments of insights and creativity as a slow growth process, are as a matter of fact not contradictory, but complementary. Gruber has made a famous study concerning Charles Darwin's creativity using Darwin's own minute notebooks and letters as a research material (Gruber 1974). According to Gruber, Darwin's various achievements were based on very slow, evolving processes of research, and *not* so much on rare moments of insight (Gruber 1981). But this does not mean that there were no insights, quite the contrary. Gruber (1981, 43) even maintains that Darwin had something like 300-500 insights per year. Not all of them were of course important, but in any case Darwin's research was full of insights. These facts suggest that creativity is purposeful and spontaneous at the same time, which is also how Darwin himself assessed his own qualities as a scientist:

"... the most important have been ... unbounded patience in long reflecting over any subject - industry in observing and collecting facts - and a fair share of invention as well as of common-sense" (quoted in Barlow 1958, 145).

So Darwin combined perseverance and industry:

"... he [Charles Darwin] had the power of keeping a subject or question more or less before him for a great many years" (Francis Darwin in Darwin 1892, 80), "... he used almost to apologise for his patience" (95)

with constantly searching for something new:

"I have steadily endeavoured to keep my mind free so as to give up any hypothesis, however much beloved (and I cannot resist forming one on every subject), as soon as facts are shown to be opposed to it. Indeed, I have had no choice but to act in this manner, for with the exception of the Coral Reefs, I cannot remember a single first-formed hypothesis which had not after a time to be given up or greatly modified" (quoted in Barlow 1958, 141).

Various models of learning and knowledge advancement, which emphasize the aspect of knowledge creation, also border on paradoxes (see Paavola, Lipponen & Hakkarainen 2001). The models of innovative knowledge communities nowadays often are based on the idea that knowledge creation is very fundamentally a collaborative and social process. But at the same time they emphasize individual initiative in processes of inquiry and learning. Individual actions are embedded in social interaction, and both of these aspects must be taken into account. The models are also based on the idea that knowledge should be seen more broadly than just as propositional or conceptual knowledge. Philosophers have traditionally emphasized the role of propositional knowledge but there have also been other alternatives. The models of innovation nowadays often refer to Polanyi's *tacit knowledge* and Ryle's *know-how* when they delineate the dynamics of knowledge creation (see Nonaka & Takeuchi 1995; Bereiter, in press). But these references do *not* mean that the role of propositional or conceptual knowledge is abandoned, quite the contrary -- the role of conceptualization is crucial in these models; but conceptual and propositional knowledge is not seen as the only form of knowledge.

### Meno's paradox - how to solve it?

A classical way of characterizing the paradoxical nature of inquiry is the *Meno paradox*. There are different ways to represent the Meno paradox, but the central idea is to problematize how inquiry in general is possible. In inquiry

"Either you know what you are searching for or you do not. If you do know, you already have it, whence inquiry is pointless. And if you do not know, you

would not recognize it even if you stumbled on it accidentally; hence, again, inquiry is impossible, pointless." (Nickles 1981, 89)

The paradox is especially interesting from the point of view of discovery. Is it possible to understand processes of discovery? James Blachowicz (1989) has characterized the Meno paradox as a question concerning *ampliative* inference. Is there any reasoned procedure from present knowledge to new knowledge, or are method and novelty two incompatible horns in a "Menoan" situation? Either there is some reasoned procedure--but then the results cannot contain anything new; or, we can get some new knowledge, but then the procedure or the method cannot be a reasoned procedure (Blachowicz 1989, 441).

An interesting version of the Meno paradox is the *learning paradox* (Bereiter 1985). It illustrates nicely the close relationship of processes of discovery and learning. The problem is to explain how it is possible to learn something that is conceptually more complex than previous knowledge. It seems that in order to understand more complex conceptual structures these same structures must somehow already be presumed:

"To put it most simply, the paradox is that if one tries to account for learning by means of mental actions carried out by the learner, then it is necessary to attribute to the learner a prior cognitive structure that is as advanced or complex as the one to be acquired." (Bereiter 1985, 202)

Starting from Socrates the idea has usually been to show that the Meno paradox is paradox only seemingly -- of course inquiry is possible, and of course we can learn also something new. But what shows the deepness of this question, is the fact that there are various, different kinds of modern solutions suggested to this paradox, e.g., the Piagetian approach (Pascual-Leone 1980), nativism (Fodor 1980), a theory of collective argumentation (Miller 1987; cf. Cobb, Yackel & Wood 1992), a question-theoretical approach (MacMillan & Garrison 1988; Hintikka 1984; Jung 1996), connectionism (Bereiter, in press), selectionist theories (Cziko 1995), or diagrammatic reasoning (Hoffmann 2000) -- to name some approaches.

I am not trying to offer any clear-cut "solution" to the Meno paradox here. What I want to argue is that this paradox can be used to back up a claim that purely rationalist and purely empiricist solutions are especially in trouble with the Meno paradox



(although arguments from the Meno paradox can also be used in order to show that either an empiricist or rationalist solution is just what is needed). According to a famous metaphor of Francis Bacon, the scientist is

"neither wholly speculative and like a spider spinning his web from his own substance, nor wholly empirical and like an ant collecting data into a heap, but like the bee feeding on the nectar it gathers, digesting it, and so transmuting it into the purest honey." (formulated by Kaplan 1964, 308)

Purely rationalist solutions end up being spun by Baconian spiders, "structure without genesis"; whereas purely empiricist solutions are proposed by those trying to manage with ant-like behavior, "genesis without structure" (Boom 1991, 274-275).

### Theory-ladenness of observations ...

The interrogative approach to inquiry is one important way of explaining how the research process can be based on bee-like activities (MacMillan & Garrison 1988; Hintikka 1984; Sintonen 1996b, 221). Menoan horns are avoided when it is noticed that we can know things in some sense and, at the same time, not know them in another sense (MacMillan & Garrison 1988, 196; Jung 1996, 19-20); so knowledge comes from a combination of knowledge and ignorance (Sintonen 1996a, 60). The interrogative approach has emphasized that prior information and background knowledge impose constraints on and, at the same time, anticipate admissible answers (MacMillan & Garrison 1988, 196; Sintonen 1996a, 60). So, the idea is that background theories and background information guide our research, but this guidance does not mean that when we have this background information, we already have answers to our problems in question (one horn in the Menoan dilemma). But this background information helps us to find answers to our problems (and to avoid the other horn in the dilemma, i.e., not knowing where to start searching for the answers).

James W. Garrison (1988) has argued that the Meno paradox shows that atheoretical scientific inquiry is impossible. Inquiry is based on some sort of foreknowledge, and this comes by theory and method. He refers to N. R. Hanson (Hanson 1972) and to the idea of the "theory-ladenness" or "concept-ladenness" of observations (Garrison 1988,

22-23). We always see, experience, and interpret things through our theories. So it is also in this sense that theories guide our research.

In the traditional hypothetico-deductive model of inquiry, inquiry is seen as theory-laden because it is emphasized that observations are important (or even possible) only when theories and hypotheses are tested (Hempel 1966). The interrogative approach to inquiry has a much wider use for theories. Theories help us to find fruitful questions, which are the basis for new information (Hintikka 1984; Sintonen 1996a). A Kuhnian "normal science" where background information and background theories guide the research does *not* mean that original scientific research is done when these background theories are engendered. It takes ingenuity to ask good questions even though basic background theories are already available (Kuhn 1970; Sintonen 1996a).

### ... and observation-ladenness of theories

What I want to argue is that the theory-ladenness of inquiry is, however, only one part of the story. What is missing is the way that observations can be clues for theories; theories are often searched for in order to explain observational phenomena. Garrison emphasizes the Hansonian (or Wittgensteinian) idea that "seeing" is always "seeing as" (Garrison 1988, 22-23). We see things through gestalts or patterns. But Hanson also emphasized that inquirers reason from surprising data *to* an explanation, i.e., they use *abductive* inference (Hanson 1972, 70-92). And one central point of Hanson was to argue that these processes of discovery ("the dawning of an explanation") have their reasons, and so they can be analyzed with philosophical concepts.

But are we not in a vicious circle? Observations are supposed to be theory-laden, and theories also "observation-laden". I think not. This is another essential tension in discovery but not a contradiction or an empty circle. Hanson tried to analyze not only how "patterns" influence the way we see and interpret things, but also the manner in which these patterns are searched for (Hanson 1965, 47-65). Although there are no "pure" observations, and although background information and background theories always influence how things are seen, this does not mean that new theories cannot be searched for with theory-laden observations. Hanson's point was that although there is

no mechanical way of making discoveries on the basis of observations (an old inductivist dream), observations and surprising phenomena operate as triggers or clues when explanations are searched for (and this suggests abductive methodology) (Hanson 1972, 70-72).

I think that this kind of a model, which would take into account that theories influence on observations but observations are also a basis for theories, would shed light, e.g., on Darwin's ideas concerning methodology. It has often been argued that Darwin had some confused ideas about methodology. For example, in a famous passage from his autobiography, Darwin said that he "worked on true Baconian principles, and without any theory collected facts on a wholesale scale" (quoted in Barlow 1958, 119). It has often been argued that this was a rhetorical argument from Darwin rather than an accurate statement of how he worked, because theories and testing of theories were so obviously starting points in Darwin's inquiries (e.g., Crombie 1960; Ghiselin 1984, 34-35). But I think that Darwin was *not* trying to maintain that he had no background theories when he made his observations nor that there could be "pure" observations which would be a basis for theories. What Darwin wanted to say was that although theories influence how things are seen, and although he often tested various theories, he also tried to collect and make such observations as would be a starting point for new and better explanations. The idea is to *try*, as much as possible, not to be influenced by previous theories, and try to make such observations as might suggest something new. This is a skill of detectives; to make (and search for) observations that are somehow peculiar and treat observations as if there were no good explanations for them. Darwin greatly valued the skill of making keen observations (see e.g., Barlow 1958, 32-39, 140-141; Darwin 1892, 94-96). And the overall picture is a constant interaction between theories and observations, as Francis Darwin depicted his father's methodology:

"He [Charles Darwin] often said that no one could be a good observer unless he was an active theoriser. This brings me back to what I said about his instinct for arresting exceptions: it was as though he were charged with theorising power ready to flow into any channel on the slightest disturbance, so that no fact, however small, could avoid releasing a stream of theory, and thus the fact became magnified into importance." (Darwin 1892, 95)

## Conclusion

I have not so much tried to find answers in this paper, but rather characterized some essential tensions in discovery. These various tensions have often resulted in dichotomies, i.e., to the idea that one has to choose between, for example, discovery and justification, or between individual activities and social interaction, or between product and process. But these tensions give also an opportunity to understand dynamic processes of inquiry. Philosophers have quite often left, or even defined, the area of discovery as something that is not amenable to conceptual analysis. I have argued, however, that there is no need for philosophers to avoid looking into discovery, and philosophical models and concepts can be applied fruitfully to help understand also secrets of discovery.

## References

- Aliseda, Atocha (1997), *Seeking Explanations: Abduction in Logic, Philosophy of Science and Artificial Intelligence*. Institute for Logic, Language, and Computation, Dissertation Series, University of Amsterdam, Holland.
- Aliseda, Atocha (2001), "A place for logic in scientific methodology: a second opportunity?", a paper presented at the *Model-Based Reasoning: Scientific Discovery, Technological Innovation, Values* Conference, Pavia, Italy, May 17-19, 2001.
- Barlow, Nora (ed.) (1958), *The Autobiography of Charles Darwin, 1809-1882*, with original omissions restored. London: Collins, St James's Place.
- Bereiter, Carl (1985), "Towards a Solution of the Learning Paradox", *Review of Educational Research* 55(2): 201-226.
- Bereiter, Carl (in press), *Education and mind in the knowledge age*. Hillsdale, NJ: Erlbaum.
- Blachowicz, James (1989), "Discovery and Ampliative Inference", *Philosophy of Science* 56: 438-462.
- Boom, Jan (1991), "Collective Development and the Learning Paradox", *Human Development* 34: 273-287.

Cobb, Paul, Yackel, Erna & Wood, Terry (1992), "A Constructivist Alternative to the Representational View of Mind in Mathematics Education", *Journal for Research in Mathematics Education* 23(1): 2-33.

Crombie, A. C. (1960), "Darwin's Scientific Method", *Collection de Travaux de l'Académie Internationale d'Histoire des Sciences*, no 12, Actes du IXe Congrès International d'Histoire des Sciences, (Barcelona Madrid 1-7 Septembre 1959,) vol I, Barcelona 1960.

Cziko, Gary (1995), *Without Miracles. Universal Selection Theory and the Second Darwinian Revolution*. Cambridge, Massachusetts: A Bradford Book, The MIT Press.

Darwin, Francis (1892), "Reminiscences of my father's everyday life", in Francis Darwin (ed.) (1892) *Charles Darwin: His Life Told in an Autobiographical Chapter, and in a Selected Series of His Published Letters*, pp. 66-103. London: John Murray, Albemarle Street.

Engeström, Yrjö (1987), *Learning by expanding*. Helsinki: Orienta-Konsultit Oy.

Fodor, Jerry (1980), "Fixation of Belief and Concept Acquisition", in Massimo Piattelli-Palmarini (ed.) (1980) *Language and Learning. The Debate between Jean Piaget and Noam Chomsky*, pp. 142-162. Cambridge, Massachusetts: Harvard University Press.

Garrison, James W. (1988), "Hintikka, Laudan and Newton: An Interrogative Model of Scientific Inquiry", *Synthese* 74: 145-171.

Ghiselin, Michael T. (1984), *The Triumph of the Darwinian Method*. With a New Preface. Chicago: The University of Chicago Press. Originally published 1969.

Gruber, Howard E. (1974), *Darwin on Man. A Psychological Study of Scientific Creativity*. New York: E. P. Dutton & Co., Inc.

Gruber, Howard E. (1981), "On the relation between 'aha experiences' and the construction of ideas", *History of Science* 19: 41-59.

Gutting, Gary (1980), "Science as Discovery", *Revue Internationale de Philosophie* 131-132: 26-48.

Hakkarainen, Kai (1998), *Epistemology of inquiry and Computer-supported collaborative Learning*. Ph.D. thesis. University of Toronto.

Hakkarainen, Kai & Sintonen, Matti (in press), "The Interrogative Model of Inquiry and Computer-Supported Collaborative Learning", *Science & Education*.

Hanson, Norwood Russell (1961), "Is there a logic of scientific discovery", in H. Feigl and G. Maxwell (eds.) (1961), *Current Issues in the Philosophy of Science*. New York: Holt, Rinehart and Winston, Inc.

- Hanson, Norwood Russell (1965), "Notes toward a logic of discovery", in R. J. Bernstein (ed.) *Perspectives on Peirce*, pp. 42-65. New Haven and London: Yale Univ. Press.
- Hanson, Norwood Russell (1972), *Patterns of Discovery*. Cambridge: University Press. First printed 1958.
- Hempel, Carl G. (1966), *Philosophy of Natural Science*. London: Prentice-Hall, Inc.
- Hintikka, Jaakko (1984), "Questioning as a Philosophical Method", in James H. Fetzer (ed.) (1984) *Principles of Philosophical Reasoning*. N.J.: Rowan & Allanheld, Totowa.
- Hintikka, Jaakko (1985), "True and false logic of scientific discovery", *Communication & Cognition* 18(1/2): 3-14.
- Hintikka, Jaakko (1998), "What is abduction? The fundamental problem of contemporary epistemology", *Transactions of the Charles S. Peirce Society* 34(3): 503-533.
- Hoffmann, Michael (2000), "Die Paradoxie des Lernens und ein semiotischer Ansatz zu ihrer Auflösung", *Zeitschrift für Semiotik* 22(1): 31-50.
- Jung, Sangmo (1996), *The Logic of Discovery. An Interrogative Approach to Scientific Inquiry*. American University Studies, Series V, Philosophy, Vol. 168. New York: Peter Lang.
- Kaplan, Abraham (1964), *The Conduct of Inquiry. Methodology for Behavioral Science*. San Francisco: Chandler Publishing Company.
- Kuhn, Thomas S. (1970), "Logic of Discovery or Psychology of Research?", in Imre Lakatos & Alan Musgrave (eds.) (1970), *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.
- Kuhn, Thomas S. (1977), *The Essential Tension*. Chicago: The University of Chicago Press.
- Laudan, Larry (1980), "Why Was the Logic of Discovery Abandoned", in Nickles, Thomas (ed.) (1980) *Scientific Discovery, Logic, and Rationality*, pp. 173-183. Dordrecht: D.Reidel Publishing Company.
- Macmillan, C. J. B. & Garrison, James W. (1988) *A Logical Theory of Teaching. Erotetics and Intentionality*. Dordrecht: Kluwer Academic Publishers.
- Magnani, Lorenzo, Nersessian, Nancy J. & Thagard, Paul (eds.) (1999), *Model-Based Reasoning in Scientific Discovery*. New York: Kluwer Academic / Plenum Publishers.
- Miller, Max (1987), "Argumentation and Cognition", in Maya Hickmann (ed.) (1987), *Social and Functional Approaches to Language and Thought*. Orlando: Academic Press Inc., Harcourt Brace Jovanovich Publishers.

Nickles, Thomas (1981), "What Is a Problem That We May Solve It?", *Synthese* 47: 85-118.

Nickles, Thomas (1989), "Heuristical appraisal: a proposal", *Social Epistemology* 3(3): 175 188.

Niiniluoto, Ilkka (1978), "Notes on Popper as Follower of Whewell and Peirce", *Ajatus* 37: 272-327.

Nonaka, Ikujiro. & Takeuchi, Hirotaka (1995), *The Knowledge-Creating Company: How Japanese Companies Create the Dynamics of Innovation*. New York: Oxford University Press.

Paavola, Sami (2001), "Abduction as a logic of discovery: The importance of strategies", a paper presented at the *Model-Based Reasoning: Scientific Discovery, Technological Innovation, Values* Conference, Pavia, Italy, May 17-19, 2001.

Paavola, Sami, Lipponen, Lasse & Hakkarainen, Kai (2001), "Epistemological Foundations for CSCL: A Comparison of Three Models of Innovative Knowledge Communities", a paper submitted for the CSCL2002 Conference (Computer Support for Collaborative Learning 2002: Foundations for a CSCL Community), January 7-11, Boulder, Colorado, USA.

Pascual-Leone, J. (1980), "Constructive Problems for Constructive Theories: The Current Relevance of Piaget's Work and a Critique of Information-Processing Simulation Psychology", in Rainer H. Kluwe & Hans Spada (eds.) (1980), *Developmental Models of Thinking*, pp. 263-296. New York: Academic Press.

Pera, Marcello (1994), *The Discourses of Science*. Chicago: The University of Chicago Press.

Popper, Karl R. (1980), *The Logic of Scientific Discovery*. London: Hutchinson. Tenth impression (revised) (first impression 1959).

Sintonen, Matti (1990), "Discussion: Darwin's Long and Short Arguments", *Philosophy of Science*, 57: 677-689.

Sintonen, Matti (1996a), "Structuralism and the Interrogative Model of Inquiry", in W. Balzer and C. U. Moulines (eds.) (1996), *Structuralist Theory of Science. Focal Issues, New Results*. New York: de Gruyter.

Sintonen, Matti (1996b), "The come-back of constructivism", *Revue Roumaine de Philosophie*, 40(3-4): 217-225.

Sintonen, Matti (1999), "Creativity and Discovery", in Kevin L. Stoebr (ed.), *Philosophies of Religion, and Art, and Creativity*. Proceedings of the Twentieth World Congress of Philosophy, Vol 4, pp. 239-247. Philosophy Documentation Center, Bowling Green State University.

Sintonen, Matti (2000), "Tradition and Innovation: Exploring and Transforming Conceptual Structures", a manuscript accepted for publication in *Science and Culture* 1.